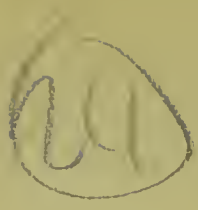

CONTRIBUTIONS

TO

ANATOMY AND PHYSIOLOGY.

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Some Remarks on the Structure and Arrangement of the Spinal Arachnoid.

A FEW years ago I published in the MEDICAL GAZETTE a translation of the memoir, by Arnold, on the Membranes of the Brain and Spinal Marrow; to the translation I added a commentary. My chief object was to point out to anatomists the existence and the peculiarities of a tissue subjacent to the arachnoid (sub-arachnoid tissue), connecting that membrane to the pia mater as well cerebral as spinal. 2. To explain my reasons for differing with M. Magendie in respect to those "communications" he supposes to exist between the fourth ventricle (cerebellar) and the general sub-arachnoid space. And, 3. To describe the true nature of that membrane which invests the cerebral and cerebellar ventricles (ventricular membrane) and the surfaces of the convolutions of the brain where they oppose each other. These observations I now make from memory, not having the memoir before me, but I believe that these were the points at which I chiefly aimed.

Since then, some additional observations on the arrangement and structure of the spinal sub-arachnoid tissue have been made by an anatomical writer, who, though evidently young and but recently in the field, has already, by the remarkable clearness of his descriptions, earned for himself a reputation which must be lasting. The observations to which I allude are contained in a foot-note to the admirable descriptive work of Mr. Viner Ellis. They are not put forward as his own, but described as having been furnished him by an old and much esteemed friend of mine, Dr. Sharpey; they refer to an anatomical structure to which, for many years, I had occasionally given a good deal of attention, and bestowed on its examination a good deal of labour. Surprised at the discrepancy between us, I repeated with, if possible, still greater care, my previous observations. To avoid every chance of error, I requested my assistants to repeat the dissections. I next made them in presence of large classes of students. After reading to them the various passages alluded to as occurring in Mr. Ellis's work, and finding that hitherto I have met with nothing to alter my previous description of this membranous structure, which, by the by, simply accords with the usual descriptions of others, I now venture to submit the matter in dispute to my brother anatomists and physiologists: the ultimate structure of these important membranes can scarcely be too frequently examined*.

Mr. Ellis's text is as follows:—"Dissection of the Spinal Cord." "A small portion of the cord is to be cut off, and the dura mater, having been laid open both before and behind, placed in water, with the dura mater pinned out on a piece of board, and with the posterior surface uppermost. If air be gently blown beneath the visceral layer of the arachnoid, the sub-arachnoid space is perceived, and it is found to be divided into a right and left portion by a septum extending along it."—P. 150.

As a lecturer on anatomy for nearly twenty years, and the conductor of large practical classes, I have been much in the habit of laying open the spinal column with the view of demonstrating the structures to classes and to individuals. Now, what I have observed having a reference to the passage just quoted is simply this, confining my remarks as strictly as I can to the

* When I commenced these observations I was in hopes of putting my hand on the "Translation of Arnold, with a Commentary," already referred to; it was published, so far as I can recollect, in the GAZETTE (perhaps in the Lancet) some four or five years ago, but I have failed in meeting with the volume, so that I am compelled reluctantly to quote it from memory.

dorsal aspect (posterior of Mr. Ellis) of the spinal marrow and its membranes. I am quite within bounds when I say that I have opened and examined the spinal marrow and its membranes, or caused it to be opened and examined in my presence, at least a hundred times.

On laying open the spinal column in the usual way, by removing the arches of all the vertebræ from the atlas to the last sacral, and incising cautiously the dura mater, the visceral layer of the arachnoid presents itself. This generally adheres by a few points here and there to the parietal layer, an arrangement which I once thought might have some remote connexion with the speculations of Goethe, Frank, Oken, Dumeril, and De Blainville, but which were supported neither by comparative dissections nor by abnormal structures. If the visceral layer of the arachnoid be now punctured, and a blowpipe introduced into the sub-arachnoid space, air may be blown in with the greatest ease, so as to fill large portions of this space, of some inches in length, and extending quite across the medulla, or, in other words, from side to side: the examination of the same aspect of the membrane under water gives precisely the same result. Thus the septum has no existence as a complete septum; its existence as an incomplete one I think extremely doubtful. It is worthy of remark, that neither M. Cruveilhier*, nor the learned authors of the Encyclopédie Anatomique, now in the course of publication, make the slightest mention of this septum on the dorsal aspect of the spinal arachnoid, nor, indeed, on any other. This is negative evidence, it is true, but of an important kind in a matter of this sort. M. Magendie's opinions must be known to these anatomists. To their works I shall afterwards return; in the meantime I may remark, that although varieties in the connexions of the visceral layer of the arachnoid (on its dorsal aspect) with the proper membrane of the medulla are occasionally met with, they amount to nothing more than the adhesion of the two membranes to each other at more numerous points by a fine filamentous and sometimes fibrous-looking tissue. Mr. Ellis continues—

"The sub-arachnoid space is much larger in the lower than in the upper part of the cord; it contains the sub-arachnoid fluid, and is divided, as Magendie has shown, by a partition along the middle line, and it communicates with the interior of the brain by the aperture in the fourth ventricle, as proved by Magendie. Dr. Sharpey considers this space to be lined by a thin membrane, that (which?) he describes as a loose serous sac; this is reflected around the cord by its visceral layer, and on the arachnoid by its parietal; it is separable from the arachnoid, and it may be raised with care, as a thin membrane, from the cord, roots of the nerves, and ligamentum dentatum. He suggests, also, that the portion extending along the posterior part of the space is formed by a reflection of this thin membrane inwards to the cord; that the sub-arachnoid fluid is secreted by this, and contained in it; and that it is continued into the ventricles of the brain, which it lines, by the aperture of the fourth ventricle into the posterior sub-arachnoid space of the brain,"—pages 150 and 151. Again, at page 14 of the same volume, "This space (the posterior sub-arachnoid space) situated between the medulla oblongata and the hemispheres of the cerebellum, is wider before than behind, and communicates in front with the anterior space by the side of the medulla and pons; on the sides and behind with the under surface of the cerebellum, and below with the large sub-arachnoid space of the spinal canal. When this space is opened, the aperture of the fourth ventricle is exposed. By this aperture the cavities of the interior of the brain communicate with the sub-arachnoid spaces both of the brain and of the medulla; and Dr. Sharpey thinks it probable that the membrane which he considers to line the interior of the sub-arachnoid space of the medulla is continued into the cavities of the brain by this opening."

In a foot-note is added—"We might conceive this membrane (the internal arachnoid) to be prolonged from the external arachnoid by the canal of Bichat into the interior of the ventricles, after lining them to be continued through the opening of the fourth ventricle into the so-called sub-arachnoid space, which it lines, being applied to

* Vol. iv. 1835.

the surface of the spinal cord, the nerves, and the ligamentum dentatum on the one hand, and on the other to the surface of the external sac of the arachnoid; the two by their mutual adhesion forming the loose arachnoid."

Now if it be meant by the above passage that the arachnoid membrane transmits into the interior of the brain by the great cerebral fissure a membranous sac lining the ventricles, then it will be quite unnecessary for me to say one word in refutation of an opinion already proved false in all our elementary works,—a statement involving in fact a physical impossibility: but the passage no doubt admits of another meaning, namely, that a serous sac lining the ventricles passes through the fissure of Bichat to communicate with the sub-arachnoid space there, and, through the fourth ventricle, to communicate at the anterior spinal sub-arachnoid space. Now this assertion equally involves a physical impossibility, or nearly so.

Before venturing to offer any critical remarks on the passages now quoted, I shall first beg the reader to remember what I said regarding the disposition of the spinal arachnoid, as high at least as the lower extremity of the medulla oblongata observed on its *dorsal aspect;* secondly, I shall state the result of several dissections of that portion of the arachnoid extending from the point until fairly traced over the cerebellum; thirdly, the dissection of the arachnoid and its space on the anterior or ventral aspect of the spinal marrow; fourthly, the distribution of the arachnoid with reference to the cerebral anfractuosities, and the great posterior cerebral fissure (so called) of Bichat, so well described by Vesalius and Galen; and lastly, I shall compare these dissections with the passages just quoted from Mr. Ellis's work.

1st. We have seen that in the vast majority of dissections air may be blown into the posterior sub-arachnoid spinal space freely; that there exists no septum here; that the points of adhesion between the visceral layer of the arachnoid and the proper covering of the medulla spinalis are extremely few. Now trace the arachnoid upwards towards the medulla oblongata and cerebellum, and it will be found that the parietal layer of the arachnoid generally adheres with sufficient firmness towards the lower end of the medulla oblongata as to prevent the passage of air upwards into that large sub-arachnoid space formed by the sudden passage of the arachnoid across to the cerebellum, leaving thus a large space immediately behind the fourth ventricle. If the arachnoid be very cautiously opened where it forms the posterior wall of this space, another membrane will be found either close to it, or separated by a considerable quantity of filamentous tissue: this membrane I believe to be a layer of the pia mater, or proper membrane of the medulla spinalis and oblongata, which quits the medulla along a line more or less distant from the margins of the calamus to reach the cerebellum by a nearer point: it varies in its position, and often seems to be merged in that deeper layer of the spinal neurilemma which, approaching the aortic margins of the calamus, passes from these to the vermiform process, and aided by the valves of Tarin, completely shuts in the fourth ventricle behind, cutting off all communication between the fourth ventricle and the posterior sub-arachnoid space. The "opening," then, of Magendie must be generally artificial. I say generally, because that a deficiency should occasionally exist in the pia mater as it shuts in the ventricle is neither impossible nor improbable. But if any additional serous sac exists exterior to the arachnoid in this space, communicating at this point with the interior of the ventricles, it can only do so by forcing its way through a real barrier which the pia mater presents against the escape of the ventricular fluid into the posterior sub-arachnoid space. This fact I have verified over and over again, and I find it constantly proved by every case of chronic hydrocephalus internus.

No mention is made of any septum in the posterior sub-arachnoid space; the internal arachnoid, as it is called, seems to commence in the interior of the ventricles whose walls it lines, communicating with the sub-arachnoid spaces chiefly at two points: 1st, at the great cerebral fissure; 2d, at the pretended foramen of Magendie. Now at the great cerebral fissure there exists a physical obstacle to such a communication, which is this: the cerebral pia mater as it passes from one edge of the fissure

to the other, shuts it closely in; it adheres, in fact, (as it does to the edges of the convolutions) to each edge firmly, passing from one to the other, and, in passing, transmits into the interior of the brain two distinct processes, namely, the excessively delicate ventricular lining; and secondly, the tela choridea and choroid plexuses. These structural arrangements were pointed out in my commentary on Arnold. Thus it is physically impossible that any serous sac can pass across the brain by the wall of the ventricles, as described in Mr. Ellis's work. The ventricular lining is in fact a layer of the pia mater, and the ventricular cavities are excluded from all communication with the sub-arachnoid space by the firm adhesions of the same membrane at the margins of the great openings into the interior of the brain, viz. the great cerebral fissure and the fourth ventricle: the prolongation of the vascular layer of the same membrane, under the name of choroid plexuses, contributes to shut up the openings.

I promised to return, thirdly, to the dissection of the arachnoid membrane, and sub-arachnoid space on the ventral aspect of the cord; and, unquestionably here, great varieties exist.

A very common arrangement is a loose adhesion of the visceral layer of the arachnoid to the pia mater, so that on blowing up the space by introducing the blowpipe, the arrangement was found to be very similar to that on the dorsal aspect. Sometimes, on the contrary, the space is crowded with filamentous tissue to such an extent, and at the same time so short and dense, as to prevent the passage of the air amongst its meshes for any great distance at least, although considerable force be used. Now between these extremes every variety exists, and it has not unfrequently happened that four or five specimens, examined in succession with a view to determine this very question, have presented each of them structures quite dissimilar to the others. To look for a serous sac here was quite out of the question; supposing it to exist in some cases (which is not impossible). Mr. Ellis has omitted mentioning how it terminates upwards.

I find that I had promised to compare these dissections with the statement in Mr. Ellis's text; but perhaps the reader had better do this for himself. Much, no doubt, remains still to be inquired into with respect to these membranes; I shall be happy to repeat the dissections with any anatomist situated within a convenient distance. It were easy for me to extend these notes, by pointing out several contradictory passages in the generally very clear and eminently descriptive work of M. Cruveilhier; but this is unnecessary. The work was published in 1835, and in a new edition they may not be found. My object is not criticism; merely truth.

In conclusion, it only remains that I should notice, with surprise, the extremely meagre and imperfect account of these structures which has just appeared in the "Encyclopédie Anatomique," a work leading necessarily to great expectations, as well from the celebrity of its translator, M. Jourdan, as from the host of distinguished and original contributors to the work, and more especially from the celebrity of that high name, Valentin, attached especially to the volume containing the "Névrologie."

On some Varieties in Human Structure, with Remarks on the Doctrine of "Unity of the Organization."

Osseous system.—The Cranium.

A few years ago there was reported in a weekly journal, but very briefly, the case of a labouring man, who, falling from a scaffolding, or having been accidentally knocked down in some way or other, was carried senseless and motionless into one of the largest metropolitan hospitals. The distinguished surgeon into whose hands this patient came having examined his head carefully, discovered on its surface a remarkable depression, and assuming this to be the probable cause of the loss of sense and motion, he proposed the application of the trephine as the most appropriate remedy. But, fortunately for the patient and for the surgeon, the patient's wife appeared in the hospital, and the nature of the operation, *with the reasons for operating*, having been explained to her, she objected to its being proceeded with on the following very sufficient grounds: viz. that ever since she became acquainted with her husband, the patient, his head had

always presented that identical depression, and therefore—but I may leave her conclusions (with which the surgeon agreed) to every sound reasoner. The patient, if I remember right, recovered without the performance of any operation. Now, I had seen similar cases before, or at least some very puzzling appearances on the surface of the cranium in persons otherwise quite well, and great difficulties have occasionally arisen as to how the surgeon ought to act; but I am not quite sure that anatomy can always furnish him with the means for arriving at a sound judgment: in two circumstances, however, I think it may assist in the arriving at a correct diagnosis,—the first great object of the surgeon no less than of the physician. An incision, as an exploratory step, made in the scalp so as to expose the cranium, may appear to many by no means a formidable operation, and this undoubtedly is true; but neither is the extirpating a trifling corn on the toes an important matter, and yet an estimable lady lost her life a few days ago in the hands of a distinguished professor of surgery in this place, in consequence of submitting to so very simple an operation.

If a considerable number of crania be examined (I mean of crania as they are preserved in museums), a certain few will exhibit the following peculiarity: along nearly the whole line of the lambdoidal or parieto-occipital suture the occipital bone will occasionally so overlap the parietal, or at least be so raised above it, as to give the false appearance of a deep depression of the parietal. This singular overlapping of these bones, or perhaps rather elevation of the occipital, I find, on examining the museum, to be by no means infrequent, and in various heads to present a great variety of appearances, agreeing, however, all in this, that, on passing the hand from the occipital to the parietal bones, a sudden depression will be felt, occasioned by the greater elevation of the former. The second circumstance in respect to such elevations at various points of the cranium is even more interesting than the first; it is of rarer occurrence, and more likely, therefore, to lead the surgeon into error. The elevation occurs in the line of the fronto-parietal suture, and is caused by an overlapping of the parietal by the frontal bone, or at least by a greater elevation of the former. Of at least a hundred crania in the museum, there is not one showing this variety in its form, at least to any extent; and a specimen exhibited by Dr. J. A. Robertson to the Anatomical and Physiological Society was the first I had an opportunity of carefully examining. What made this case peculiarly interesting was its occurrence in a person who had met with a severe injury to the head, which ultimately proved fatal. The *seeming* depression close to the fronto-parietal suture was so great as nearly to induce the attending surgeon to apply the trephine; but the death of the person taking place very soon after enabled the medical attendant to discover the very serious error into which he must have fallen had he persisted in his intention of trephining: the real seat of injury was through the base of the skull, with fatal effusion of blood, altogether beyond the reach of the operator.

Before adverting to other varieties in form met with on the surface of the cranium, I may briefly notice the German transcendental theory, which endeavours to explain these varieties, to reduce them to a law, the law of "deformation," and to bring them within the range of the philosophy of animal beings. The theory, which must be well known to most of my hearers is, that these "deformations" simply reproduce a structural form, which, however rare in man, and therefore in him "abnormal," will yet be found to be the regular law in many animals placed lower than man in the scale, and therefore are in them quite regular, or "normal." But why should they appear in man at all? This difficulty the German theorist meets by reminding you of another law, viz. unity of organization: that in the embryo there is unity of organization: that the embryo of most animals runs through certain phases or "developments:" that the earliest of these developments resemble, therefore, certain animals, and the latest, others: that should the change, from any cause whatever, known or unknown, not take place, there is then an imperfect development, "*arrêt de développement*," and that this may take place at any stage, giving even to the human form the peculiar arrangement of parts belonging to a lower class of animals, or retaining in the adult the

embryonic form of its own organs. To apply these laws to the peculiarities in question, we have only to say with the German, that the human cranium, which, when fully developed, ought to present a uniform and comparatively smooth surface, and a series of immoveable sutures, has, in the fœtus, a totally different character; in it, the sutures are moveable, and slide freely over each other, in short, may be made to overlap each other; that when dried, prepared, and deposited in our museums, the fœtal head actually presents these appearances; that should the adult head present a similar form in some, it is merely an "*arrêt de développement*;" and finally, this very irregularity, by the great law of unity of organization, is the usual normal form in many species of the lower animals: in them the "arrest of development" is the law; in man it is the exception.

I shall afterwards endeavour to show that these are the only views which, in the present state of science, can be adopted, and that what has been written against them in France, and more especially in England, is simply, and, to use the mildest phrase, ingenious nonsense; sometimes very pompous and imposing, as in the Bridgewater Treatises, but still downright nonsense, and not meriting the smallest attention from any philosophic mind. The German theory, or rather the theory of Goethe (for it is his), requires very considerable modification before it can be applied extensively to the history of organization. Let us proceed, in the meantime, with details.

On examining the museum to find the crania of those animals which ought, according to the theory, to present the peculiar form or forms above described as "anormal" in man, but "normal" in them, it is not so easy to find them. That such exist I have no doubt, but the examination of some hundred specimens of the crania of different mammaliæ has not furnished me with any clear illustrations of the law. By descending lower, however, in the scale, we must meet with them; the cranial bones of the cetacea overlap each other at many points; and so more especially do those of fishes. The law of unity admits, nay requires, that we should include the entire scale in the inquiry after truth.

If we do not work with great numbers, and on an extended scale, the theory cannot be supported on all occasions: it is a psychological and metaphysical theory, as well as a physical, and they who do not admit this, either do not understand it, or are unwilling to be informed as to its truth.

Of other appearances met with occasionally on the outer surface of the cranium I shall mention but a few. There are first those ridges, single or double, running in the line of, or parallel to and at a little distance from, the parietal suture, remarkably developed in a skull-cap now before me. These elevated parietal lines or cristæ are simply the *superior parietal crests* generally not perceivable in the human cranium; at other times, as in this instance, quite distinct. They are of course quite common in many of the mammalia, but are only seen in some men. They may occupy the proper parietal suture, as in some animals, or be a little apart: they must not be confounded with the *inferior parietal crest* running continuously with the external frontal crest (also distinct enough in some men), and which, from the narrowness of the parietal bones in some mammals, approach close to the vertex, enlarging thus greatly the temporal fossæ: the crests I here allude to are different from these temporal crests, but may coincide in some animals: in man they do not, and this is owing in him to the great breadth of the parietal bones. But the crania of mammals present every variety in respect to the extent, direction, elevation, and number of these occipital, parietal (superior and inferior), and frontal crests, whose outlines may all be seen on man, fortunately but little developed in him, the "arrest" being in him happily the law; the full development, on the contrary, being the law in the highest of the quadrumana.

It has always appeared to me that these crests, although they be but imperfectly developed in man, ought to be named in accordance with their real nature and meaning; they have little or nothing to do with the history of the cranial sutures, whose course, indeed, they do not generally follow: the temporal crests of the frontal and parietal bones are continuous, running directly across the suture, which they do not regard; the occipital crest is on the occipital bone, and not in the lamb-

doidal suture, and the superior parietal crests when found in man do not seem to me to run in the parietal suture, but parallel to it; in many animals, however, there is a parietal crest continuous with the anterior branch of the occipital crest, directly in the line of the suture. The reason why such crests should be found on the surface of the cranium in different animals it seems to me impossible to say; even in my youngest days I had always philosophy enough in me to laugh at the Bridgewater nonsense (a theory all but exhausted by Philo, Derham, and Paley), which would assign to them the mechanical reason for "extending the attachments of certain muscles."

There is a single remark more I beg leave to make in respect to these crests; they are not more fully developed in the darker race of men, than in the Saxon, Celt, or Pelasgian races: at least it appears so to me from all the specimens I have examined: if we are, then, to denominate them an "arrest of development" (although this is clearly a misnomer in one sense), it ought to be kept in view that the occurrence is in no shape peculiar to the dark races of men.

Occasionally on the surface of the cranium we meet with an absolute depression, sudden, and as if scooped out at the expense of the outer table and diploe alone; nothing can be more likely to mislead the surgeon. I find them in crania which appear to me otherwise quite healthy, but still they may have been occasioned by a blow inflicted when young — even by the forceps of the accoucheur. If my recollection be correct, I think I remember a cranium which was presented by Sir G. Ballingall to the College of Surgeons, whose surface was completely notched and indented with blows; it belonged to a native of Australia. On the other hand, these depressions occasionally correspond with exostoses found projecting from the inner table, which possibly during life may have given rise to much distress.

There is a skull-cap now before me, and I can produce several others, in which such remarkable appearances are not limited to the exterior table; in this cranium the anterior half nearly of the inner table is rough, with sharp elevations; the bones are thickened generally, and at two points absorption had commenced in the inner table, and all but perforated the bone, and still there is nothing visible on the outer table as regards this remarkable change in the interior of the frontal bone. What may be observed on the outer table is a prominence of the parietal sutures, and an elevation of the outer wall of the left frontal sinus: the prominence of the bone at this point during life would have strongly resembled an exostosis; and yet it is not so, but merely a general thickening of the outer table and diploe, extending from over the sinus to beyond the external angular process of the bone.

The last variety I shall allude to is that peculiar deformity, for it must be so called, which consists in a want of symmetry in its various parts.

About a year ago, a naval surgeon, who had formerly studied anatomy with me, had the obliging kindness to present me with a remarkable cranium; being no less than the cranium of one of the party who attacked and slew our immortal circumnavigator, Cook. This cranium, which I now place before my audience,* unquestionably belonged to a native of the Sandwich Isles; its resemblance to those crania met with in museums as Peruvian and Brazilian, is remarkable, more especially in respect to its want of symmetry, thus shewing an affiliation between the aborigines of the mainland of America and those of certain islands, but not of others, as of Van Diemen's Land for example, or Australia: the crania of the natives of these islands appeared to me different from that I now speak of. I shall consider the want of symmetry more in detail in a special lecture.

II.—*The humerus: its supra-condyloid process.*

Comparative anatomists had long ago described a foramen, or short canal, traversing the humerus somewhat obliquely in carnivorous mammals, and they had also described, what could not well escape the most casual observer, that this canal was situated a short way above the inner condyle of the humerus; that it was formed by a process of bone running from the shaft to the inner condyle, and that the humeral artery and its accompanying

* These contributions were mostly submitted to my anatomical class at various meetings.

nerve, the median, in its descent to the bend of the elbow, first passed behind this process, thereafter returning to their original course, or nearly so, by traversing the canal thus formed. Physiologists, as usual, offered a mechanical reason for this deviation in certain animals from the more common structures, and, as usual, their reasons were destitute of common sense. It is not even worth while adverting to them. They resembled Sir A. Cooper's reason for there being two mammæ, and Bartholin's reasons for the male having rudimentary breasts, *ne gloriatur femina*, &c.; it is a vile patchwork, almost peculiar to British physiology, a jumble of expedients and contrivances to meet difficulties.

The possible occurrence of such a process as a supra-condyloid in man, accompanied by a corresponding deviation in the course of the artery and nerve, was to be foretold, according to the theories of Goethe, and was, perhaps, perfectly well understood in Germany, but certainly not in this country. I do not believe that any similar preparation to the one now before you exists in Britain. It is not my intention to describe it at any length here, having already done so in a memoir read to the Anatomical and Physiological Society, and printed in the Edin. Med. and Surg. Journ. for 1841. All I mean to do is simply to notice it as a process found on the humerus, a short way above the inner condyle, of varying length, tending downwards towards the condyle, and connected to it by ligamentous or aponeurotic fibres. In most humeral bones I have examined the process is entirely wanting, or nearly so, its absence being a specific human characteristic; in others, human also, there exists a rudiment of it, with a groove above and below it; and in the one before me the process is at least half an inch in length; the artery and nerve pass behind it, and the structures are extremely analogous to those of the panther, tiger, cat, &c. It will not be objected, I trust, to this view that the process is incomplete, seeing that it does not reach the condyle. The course followed by the artery and nerve will form the subject of a few remarks when I speak of the varieties of the human arteries, and referring, therefore, my audience to the memoir already published by me on this matter, I shall confine myself to a single additional remark.

However well known the doctrines of transcendental anatomy were to Goethe, Frank, and others in Germany, there were still some first-rate anatomists, also German, who, about that period, and for some time afterwards, did not rightly comprehend them. Amongst these, it is surprising to find the name of the illustrious Tiedemann, the first of living anatomists. In his great work on the Human Arteries, a very splendid engraving is given of a variety in the course of the interosseal artery, which arose in this case from the humeral artery about the middle of the arm: quitting the course of the larger vessel, the interosseal descended towards the internal intermuscular ligament, or partition, and passing behind a supra-condyloid process represented in the engraving, fully as well developed as in the specimen on the table, it afterwards regained its position in the bend of the elbow, ultimately following the course of the interosseal. This supra-condyloid process, as it really and truly was, with its ligamentous band tying it to the inner condyle, M. Tiedemann calls an "excrescentia ossis humeri in solita."

III.—*Osseous system.*

The comparatively rare occurrence of pre-sternal bones in man, more especially perhaps in this country, induces me to publish the following observations respecting them. During the whole period in which I have superintended rooms for teaching practical anatomy in this city, I have never had occasion to observe in a single instance the presence of pre-sternal bones in men, although nearly all the more rare forms of the human sternum have come under my notice; until a few months ago, when describing to my morning class the anatomy of the thorax and its contained viscera. The person in whom these bones occurred was a stout muscular man of a stature exceeding six feet, and about 25 years of age; his general shape was somewhat peculiar, and this, added to a remarkably swarthy skin and black hair, induced me to suppose that notwithstanding his Saxon name, the individual was really of the Celtic race.

But be this as it may, I requested the gentlemen engaged in the dissections to

look carefully for any variety in form which might occur, and one of the first was noticed by myself in examining the sternum. Whilst describing the superior aperture of the thorax still covered with soft parts, I remarked to the class that although the sternum was sufficiently broad in most parts, it presented at its *tracheal incisura* or notch a peculiar arrangement, which led me to suspect the presence of an anomalous structure.

Soon after the lecture I examined into what this anomaly might be, and found the narrowness of the superior portion, or rather margin, of the sternum to be owing to the presence of two episternal bones, occupying precisely the position in which they were first noticed by Beclard,* and afterwards by M. Breschet,† and by Mr. King‡ of London.

The specimen I examined with a good deal of attention, but found little to add to the extremely accurate descriptions of preceding observers: situated behind the sternal attachments of the sterno-mastoid muscles, and mesially in respect to the articular surface for the clavicles, the pre-sternal bones are attached by their bases to the inner or deeper margin of the notch of the manubrium of the sternum; they are of a pyramidal form, and approach each other slightly at their summits. The base of each appeared to me encrusted with cartilage, and there existed a close but distinct moveable joint, with a synovial apparatus, and strong ligamentous bands of a peculiar reddish colour, between them and the sternum; one was less moveable than the other, and a ligamentous band connected them to each other. A few muscular looking fibres, but extremely short, ran from the sternum to these bones.

It is not my intention to enter upon the much disputed ground as to the nature and true signification of these pre-sternal bones; Beclard fancied they might be the rudiments of the fourchette (clavicule furculaire), fully developed in birds, and reduced to a mere rudiment, and that too only occasionally present, in man; but this idea, however plausible, has not been generally adopted by anatomists: I know not, indeed, that any philosophic anatomist coincides with this view of Beclard's. M. Breschet's opinion is, that the sus-sternal or pre-sternal bones represent, or are, in man, the rudiments of a cervical rib, of which the vertebral portion is very usually found in connection with the 7th cervical vertebra, and was first described by Humauld; and the *sternal* portions of these ribs M. Breschet supposes to be represented by the episternal bones.

Having made extensive researches into the history of these cervical ribs, I am aware of certain difficulties which are opposed to the adoption of these views of M. Breschet. But whatever may be the ultimate determination in respect to the real nature of these bones, it is surely more philosophic to suppose them "rudimentary" of some structure more highly developed in some other class of animals, than to adopt the "Bridgewater" and "Guy's Hospital" physiology, which argues that every animal is made for itself alone, stands alone, and has nothing to do with any other, and that the individual organs of man and animals are to be explained by a physiology whose highest stretch of generalization is to represent the mammæ of the human female as having been purposely created double, that the accidental loss of one, by milk abscess, or otherwise, might occasion no interruption to their function!* Profound philosophy! but proving, at the same time, to how little purpose John Hunter lived and laboured, and bequeathed to Britain his immortal museum, seeing that into the educational institutions of his adopted city he failed to introduce a single spark of his philosophy.

IV.—*Muscular system.—Varieties in muscles; musculus-hepatico-diaphragmaticus.*

Muscular system.

In the same person whose sternum presented the pre-sternal bones, I discovered a muscle in a situation altogether unexpected and extraordinary. I shall call it the musculus hepatico-diaphragmaticus, or M. diaphragmatico-umbilicalis, both names expressing in part its singular course and connexions. The dissection of the part was made in presence of, and explained to, the anatomical class. I copy the description dictated by myself at the time to Mr.

* Mém. sur Ostéos,e p. 83.
† Annales des Sciences Naturelles, 1838, p. 91.
‡ Guy's Hospital Reports, Vol. 5, p. 227.

* Cooper on the Mammæ; Bridgewater Treatises, *passim*.

Morries, one of the most talented students.

This anomalous and very singular muscle arose, or was connected by a broad tendinous base to the cordiform tendon of the diaphragm, about an inch to the left of the gullet. It was connected with the diaphragm in two ways, or by two slips or attachments: the superior slip terminated shortly in a tendon which descended perpendicularly, or in the axis of the body; the inferior by a series of tendinous fibres which came directly from the tendinous fibres of the diaphragm itself (left portion), and of its cordiform tendon. The muscle or muscular band thus formed by these double origins was about an inch and a half in breadth; it had the same muscular character as the diaphragm itself. Proceeding from left to right, and gradually narrowing, it crossed the middle plane of the body, crossing in succession, 1st, the small portion of the left crus of the diaphragm; 2dly, the gullet; 3dly, the inner portion of the right crus of the diaphragm. Hitherto, the muscle in its course lay close to the concave surface of the diaphragm, but now, descending a little, it subdivides, at its origin, into two parts, a smaller and larger. The smaller, which was also the descending portion, seemed at first disposed to follow the direction of the right crus, but was soon lost, for after a course of about an inch it terminated by gradually disappearing on the outer surface of the peritoneum; the larger or stronger portion, rather more than half an inch in breadth, proceeded horizontally until it reached the concave surface of the liver, terminating in that sulcus which contains the remains of the ductus venosus. To the lower edge of the obliterated duct the muscle adhered by a series of tendinous fibres, and these might be traced not only upwards to the point where the duct comes off from the sinus of the portal vein, but onwards until it also adhered to the remains of the umbilical vein throughout a considerable space. The broad end of the muscle, near its origin, was now cut through, when a series of short muscles presented themselves, arising by tendinous fibres from the cordiform tendon, and terminating in the same tendon, but something more to the right. All these muscular slips proceeded over the margin of the œsophageal opening.

Here was a new attachment, then, of the liver and its vessels to the abdominal walls.

The usual decussation of the diaphragm was next examined; a strong band of fleshy fibres passed from the left to the right crus, but were in the opposite direction, properly speaking; a small portion only of the right crus lay over the above, but descended no further than the middle plane of the aortic opening.

On examining the remains of the umbilical vein left attached to the liver, the portion more immediately connected with the vena portæ was found enclosed in a sheath of fibres, having all the appearance of muscular fibres; this extended for about 2½ inches; further than this I had no opportunity of tracing it, the parts beyond having been destroyed by a *post-mortem* examination. During the same examination, the abdominal viscera had been removed prior to my own dissection of the above structures.

Future inquiries will, no doubt, some day prove the muscle I have just described, as well as the muscular sheath of the umbilical vein, to be connected with the doctrine of "unity of organization" in the animal series, or in the history of the human embryogenesis.

Varieties of Arteries.—1. *Humeral and Interosseal.* 2. *Subclavian.*

I shall first notice a variety in the relation of the humeral artery—perhaps the most remarkable which has as yet been recorded in respect to this artery.

The variety to which I allude is that deviation of the main trunk of the brachial from its usual course, to pass behind the supra-condyloid process of the human humerus, accompanied by the median nerve, and after descending for a short way in the supra-condyloid groove regaining its normal position and course near the bend of the elbow. By a reference to my memoir on the supra-condyloid process in the human humerus, published in the Edinburgh Medical and Surgical Journal for 1841, it will be seen, that the artery and nerve, by following this course, reproduce most exactly the normal arrangement in the arm of the tiger, panther, cat, lion, and most carnivorous animals. The authors of the Bridgewater Treatises, and their supporters in "Guy's"

and elsewhere, are, I think, bound to offer some explanation of these varieties, seeing that they object in toto to the doctrines of "unity of organization," and, indeed, to the transcendental anatomy of Frank, Goethe, Dumeril, and St.-Hilaire. Some plausible reason should in justice to their views be offered why the humeral artery and the median nerve should in some few individuals of the human race follow the non-human arrangement; there must be some reason for this, and since they object to the doctrines of "unity of the organization" they are in justice bound to offer a substitute.*

Since writing and publishing the memoir just alluded to, I find a variety in Tiedemann's great work on the arteries somewhat analogous to the above, but affecting the course of a different vessel. In this case the interosseal artery arose from the humeral artery, about the middle of the arm; quitting the course of the larger vessel, the interosseal branch made its way towards the internal inter-muscular ligament, and, passing behind the supra-condyloid process, which M. Tiedemann calls an "excrescentia ossis humeri in solita," it afterwards regains its position in the bend of the elbow. No mention is made of the course of the median nerve in this instance, but it may be presumed that it followed its usual course. M. Tiedemann adds, that this variety had been seen by Ludwig, Sabatier, Monro, Hilldebrandt, and Barclay.

2. The next variety affected slightly the course of the subclavian artery, but was also connected with a variety in the arrangement of the scalenus anticus muscle. This muscle, on the right side, in the stout muscular adult in whom the pre-sternal bones were observed, divided into two portions, betwixt which the subclavian artery passed across the rib: the variety seemed to me an approach to that other more important variety in the course of the subclavian, first observed, I believe, by a Parisian student, and pointed out by him to M. Cruveilhier; I allude to the reported passage of the subclavian artery altogether anterior to the scalenus anticus muscle, and close to the corresponding vein: Mr. Spence assures me that he has seen this most remarkable variety *once;* the nearest approach to it I have myself observed is the passage of the artery through the scalenus, as above.

The anatomy of the osseous pelvis and its connecting articulations has naturally been very frequently brought under my notice during the last twenty years, both as a teacher of human and of comparative anatomy. To the human pelvis chiefly has my attention been directed, without altogether neglecting that of other mammals, and to it the following remarks will chiefly, but not exclusively, apply.

The observations I have already published on the form of the pelvis as predisposing to hernia in certain persons; on its want of symmetry in connection with the same subject, and as determining the hernia on one or other side: these observations I need not here allude to, further than referring the reader to the Edinburgh Medical and Surgical Journal, in which he will find them: the memoir bears the title of "Observations on the Statistics of Hernia." The memoir was criticised by M. Malgaigne in a memor published by him in the Annales d'Hygiène: I presume he meant to be fair enough, and therefore I do not intend replying to his memoir further than by referring those who understand the language in which my observations were written, to the observations themselves, where they will readily enough discover, 1st, that M. Malgaigne has mistaken my meaning in all the essential points; 2d, that he has given me no credit for exposing first, I believe, the fallacy at that time exceedingly prevalent, that hernia was a very common disease*; 3d, and (this I complain most of, if it were worth while,) that he has appropriated to himself an idea of mine with such dexterity, as to make it quite his own; the idea was a very simple one, but I do not remember having met with it in medical works; it was simply this, that different races of men may be liable in varying ratios to the

* When my brother discovered the curious fact, that in the knee-joint of the ornithorynchus paradoxus the synovial membrane stretches completely across it, forming a partition between the superior and inferior parts of the joint, or in other words two joints, I asked the distinguished author of the Bridgewater Treatise "On the Hand" what purpose he supposed such a structure could serve; he replied that he thought it must be to strengthen the joint!

* This fallacy was supported by Monro, A. Cooper, and a host of the first names in the profession: I believe I was the first completely to expose the error.

surgical disease—hernia. Having got hold of the idea, new to him no doubt, and therefore "piquante et nouvelle," it is incredible how long and loudly he dwells on the chime. But to return.

Variety in form of the human pelvis.

Scarcely any part of the skeleton presents a greater variety in its form than the pelvis; admitting, however, that every part of the skeleton does present its own deviations from the usual or more ordinary form. Yet in the pelvis these deviations are extremely frequent. As these varieties in form, however, are not necessarily hereditary, and are checked by intermarriage with different families, they never proceed to the length of establishing any permanent variety, either in regard to the general form, or in regard to a specific form of any particular part; moreover, against the perpetuating of such varieties, otherwise than by their so frequent reappearance, there exists the great physiological law of species, or of regular formation, by which all the more serious deformations are at once checked in the non-productiveness of the individual. Thus, whatever the varieties in the form of the human pelvis may be, of this we may be assured, that as yet they have failed in establishing any permanent deviation in form from the human specific form; the *law of species* is the antagonistic force to the "deforming powers or laws."

Weber's theory, that in the various races of men the *female pelvis* presents a specific form bearing a fixed relation to the form of the head of the child and the future adult, is a theory based on a final cause, and not on transcendental anatomy and physiology. It comes from a quarter (Germany) where we should least expect it, and involves another theory not chiming in with it, viz. that there are four or five specifically distinct races of men. I shall leave Weber to answer these objections, for they are so; the doctrine of final causes being unphilosophical when employed as a substitute for a philosophical theory, and it is not proved that there are four or five distinct races of men and of women whose heads and pelves are specifically distinct, remaining so throughout all ages. The few specimens we possess in those European museums I have examined of the female pelvis in the various races of men, do not furnish at all any positive grounds in support of Weber's theory, and the varieties specified by him will I think be found to occur in all the races indiscriminately. The crania of the mingled European family have almost every form; so also has the shape of the female (European) pelvis: the rounded pelvis of the Japanese female, with all its dimensions nearly equal, is not peculiar to that race; nor do we find in the negroes that peculiar elongated form which this theory would lead us to anticipate; not at least so frequently, or so exclusively, as to warrant us in ascribing it to that race as a specific form.

Varieties in the form of the pelvis, and its component parts.

The most important in a practical point of view are the *sexual* differences; but these have been so well described by so many anatomical writers as to render my saying anything regarding them here almost unnecessary. I allude to the sexual differences when fully established, that is, in the adult; what I have to say on these points will arrange better with some other sections. Not unfrequently specimens are met with which at first sight are difficult to be decided on in respect to the sex; but attentive and careful observation will, I believe, always enable the anatomist to do so, particularly if he applies the foot-rule. I have seen some good practical anatomists puzzled for a moment when they attempted a hasty decision.

The different parts of the pelvis follow their own laws and formation, that is, they are not necessarily made in absolute dependence on each other: the true pelvis or excavation (cavum) may be ample for all sexual purposes, as in so many of my country-women, both English and Scotch, and yet the haunches appear remarkably narrow; this is owing merely to the fact, that the true pelvis and the false pelvis not being necessarily developed in the direct ratio of each other, but often the opposite, the false pelvis may be narrow, wall-sided, and its walls nearly upright, giving a remarkable want of breadth to the haunches; whilst the true pelvis or cavum may have, and generally has, the dimensions required by nature for the due performance of the sexual functions. The opposite form to this sometimes takes place; it gives a false

appearance of breadth and capacity to the female haunches, and may lead the accoucheur into error.

Transcendental and other physiological laws as applicable to the pelvis and its varieties.

A portion of the pelvis, a single bone, a section, a half, mesial or horizontal, may all, or any of them, be fully developed, and not the rest. Its pubic portion, which may be viewed as the pelvic sternum, may be wanting, and this coincides with deeper malformations, affecting the bladder and genital organs; but the laws of whose application I mean here to speak more particularly are, 1st, the law of unity of organization in the animal kingdom, and the coincidences of that law with the embryonic or rather fœtal structures and forms. There will be no occasion for my following any systematic order in describing the facts, or stating what they illustrate.

The posterior wall of the pelvis is formed by a continuation of the vertebral column; the mere anatomist describes the pelvic portion of the column as composed of sacrum and coccyx, not venturing to give them other names to the student, lest, what is most likely, the student should misunderstand him: all true anatomists know that this section of the column comprises at least two regions, sacral and coccygeal: that these vertebræ, nine in number, which ought, perhaps, to be differently classed, are even by the coarsest physiologist considered as distinct; as divisible, in fact, into vertebræ of two distinct classes, namely, sacral and coccygeal. But may there not be here three classes of vertebræ, of one of which the first coccygeal bone may be the representative confined in man to one, but in most mammals extended to many bones? Or has a class of vertebræ been struck out altogether? In the neck of man and of mammals generally there would seem to be three distinct classes of vertebræ, of which, in man, the first comprises the atlas and dentata; the second comprises the 3d, 4th, 5th, and, as I think, 6th; and the third class in man has in it only one vertebra, namely, the 7th, or proeminens; whereas, in the sloth, this same region comprises two or three vertebræ carrying ribs, as the 7th in man so frequently does. Now it is by no means unlikely that the same may happen in regard to the pelvic portion of the column; a whole class of vertebræ may be left out, or represented by a single one—the first coccygeal.

Of these vertebræ I shall consider, first, the varieties in form of the three inferior, or the 2d, 3d, and 4th coccygeal. These probably belong to a class; in early life they are perhaps comparatively large, and even may be observed to deviate to one side or other from the mesial plane; this deformity takes place very early. They vary in shape and in number, and in the muscles attached to them: twice only, in all my life, have I observed the presence of a distinct *sacro-coccygeus muscle;* but in these it was large and distinct, and admitted of no sort of doubt*. Their variety as to number must be rare; I do not remember having met with an instance of it. On the varieties of the first coccygeal vertebra I need not dwell: they refer principally to size, to its osseous union with the adjoining vertebræ, and the greater or less development of its pedicle and arch. It partakes a little in that "arrest of development" of the half of the pelvic portion of the vertebral column connected with the pelvis oblique ovata of Naegele, of which remarkable deformity, and its explanation on the principles of transcendental anatomy and physiology, I shall speak presently.

Sacral vertebræ.

Anatomists had laid down discriminating characters for the male and female sacrum as a whole, that is, after the invertebral ligaments and soft parts had disappeared, and the five vertebræ had become united into one. They described the female sacrum as broader, shorter, and more concave, than the male; and this, I think, will still prove the correct opinion as to the greater number. But I quite agree with Mr. South as to the fact of there being great varieties in all these matters.

Do the sacral vertebræ belong to all

* In 1821, whilst dissecting in the practical rooms of La Pitié, I accidentally met with a perfectly distinct case of a tendinous intersection of that portion of the orbicularis palpebrarum which lies over the temporal aponeurosis and external angular process of the frontal bone; it divided the muscle into two equal parts. I have never met with this tendinous intersection since, and regret the not having preserved it, as I find its occurrence denied by M. Cruveilhier.

one category? it appears to me evident that they do not. The first sacral and last lumbar occasionally so strongly resemble each other, that they can scarcely be distinguished from each other; the first, second, third, and fourth, partially form alone the articulation with the os innominatum; the fifth has nothing to do with it. They have different relations, then, to the adjoining structures, and moreover, it is generally understood or admitted that besides having all the elements of true vertebræ, though called false, they combine in addition a series of rudimentary ribs undistinguishably united by bony union with their transverse processes. There is every probability that, in addition to very large processes, these sacral vertebræ do carry upon the anterior surface of these processes rudimentary ribs; but still there lies a difficulty in fully coming to this conclusion. It is this. When Meckel published his work on General and Descriptive Anatomy (a translation of which, by Jourdan, shortly afterwards appeared, and from which translation into French I quote), he maintained the doctrine, that the thoracic ribs were merely the fully developed anterior roots of the transverse processes of the dorsal vertebræ. Now this theory, so soon as announced, I showed to my class must be incorrect; in fact, there were preparations in the museum which refuted it: one, in particular, of a seventh cervical vertebra, where the transverse process was distinctly double, that is, had an anterior and a posterior root, with a considerable aperture between them for the passage of the vertebral vein; in front of the anterior root was another aperture, and then came a cervical rib; thus, there could be no doubt of Meckel being in error in respect to his theory that the ribs are merely the prolonged anterior roots of the transverse processes of the vertebræ. Now to apply this to the human sacrum, it is easy to show many specimens of the young bones where each lateral mass of each sacral vertebra is growing by *two distinct germs;* but whether these germs represent the one the transverse process, the other the rib, as in the dorsal vertebræ (in which vertebræ the anterior root has entirely disappeared), or whether these two germs on each side represent merely the anterior and posterior roots, or whether both are present and rudimentary ribs also; these are questions which the preparations I have as yet examined do not enable me satisfactorily to solve. An appeal to comparative anatomy is only one way of solving the question, and not a very satisfactory one when applied to human structure, and this for the most obvious reason in the world.

The sacral vertebræ present, as we have seen, the greatest caprice as to their form, without, however, deviating very much from the normal arrangement; sometimes, however, they do: first, as to number; secondly, as to "development" of their natural halves. Of the variety as to number I shall say nothing: six vertebræ have, no doubt, been found, but on all such occasions it would be well to look to the number of the lumbar vertebræ. Secondly, the variety in the development, or rather the non-development, of the lateral halves of the sacral vertebræ, is one of the most important deformations to which the pelvis is liable. Its explanation probably rests on a law in transcendental anatomy, or rather physiology, which I shall first state.

If the pelvis of a fœtus at full term, or before or after for a short period, be examined, it will be found to differ entirely from the adult pelvis, *and most especially from the finely-formed European female pelvis;* it resembles, also—a fact which is well known to the transcendental anatomist—the pelvis of the lower mammals, whether male or female. To them it bears the strictest resemblance in as far as the structures will admit. I shall describe the particulars of its form more minutely afterwards, merely remarking here that the introitus, and, indeed, general form of the cavity, is an elongated square rather than an oval; the sides, formed by the ossa innominata, are nearly straight; the cavity is of equal breadth throughout, and the antero-posterior measurement of the introitus is the larger; any one may recognise in this the pelvis of the lower mammals. Now, should the pelvis during its development continue of this shape, or even maintain it to a certain extent, then we have a misshapen pelvis, common enough in females both here and on the continent: the pelvis continues, to a certain extent, to maintain its fœtal

form; there is a kind of arrest of development; the sides are too straight, the sacrum too narrow; the transverse diameter of the introitus too small, and the antero-posterior diameter too long. I repeat, pelves of this form are exceedingly common in the male; they are also common in the female.

But let us suppose that the arrest of development has been limited only to one side, confined to the left half, say, of the sacrum and corresponding innominatum, but that the arrest of development has been here, if I may so say, much exaggerated, that is, more complete; that the bones have not only not undergone those changes as to *form* which they ought to have done, but that moreover they are positively much *smaller* than those of the opposite side; that, in fact, that has taken place here which we shall find may take place even in the ribs. Then there is produced the "pelvis oblique ovata," the discovery of which is due to the illustrious Naegele. A glance at fig. 1, pelvis of fœtus; fig. 2, of mammal; fig. 3, of ill-formed female pelvis, but quite capacious enough for the passage of the child; fig. 4, the pelvis oblique ovata of Naegele; fig. 5, the finely-formed female pelvis: a glance at these figures will best explain the whole theory to the reader. Dr. F. Krammerscroff, of Dorpat, whom I had the pleasure to meet lately, a most intelligent observer, informed me that he saw lately, in the museum of Dr. Outrepont, a female pelvis in which the deformation now known by M. Naegele's name of "pelvis oblique ovata," extended to both sides, giving rise to the greatest difficulty during labour, and ultimately, if I rightly recollect, causing the death of the patient. If it were not that anchylosis or bony union of the sacro-iliac joint, so generally, if not always, coincides with the arrest of development, giving rise to this deformity, Dr. Outrepont's case was one in which the pubic section might have been attempted with some slight chance of success; to propose this operation in the round or transversely oval pelvis argues a want of mere mechanical knowledge on the part of the adviser.

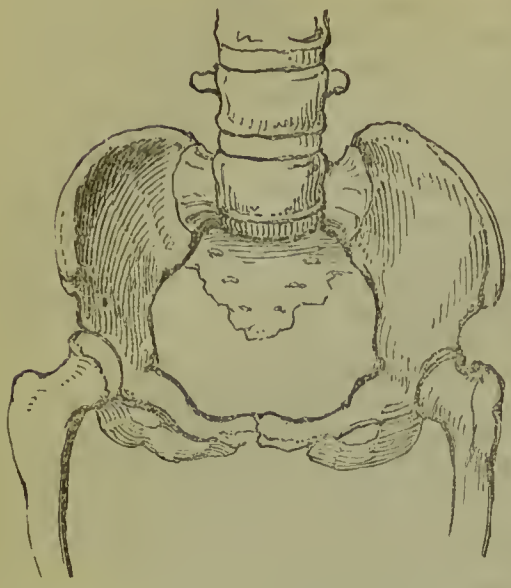
The transcendental theory I have just endeavoured to apply in explanation of, 1st, the normal form of the male pelvis and its varieties; 2d, the form of the pelvis of the mammal; 3d, certain unusual forms observed in the female pelvis must ultimately rest on certain facts which it is proper to examine more in detail, or rather by direct or intuitive perception. If these be correct, then the theory is good so far as it goes: it gives us no insight, it is true, as to the *why* these things should happen so, but it shows the *manner* of their taking place. The details I shall now take the liberty of giving are taken chiefly from scattered memoranda of lectures.

The resemblance of the abdomen, pelvis, and trunk generally of the human fœtus and infant to the same parts in the adult quadruped mammal, is very striking; it depends, no doubt, on the law of unity of organization. But I may as well quote the notes as they stand in my note-book.

If the principles and views just laid down be in accordance with nature, they ought to stand the test of intuitive inspection, or an appeal to individual phenomena; let this appeal, then, be made to the human and brute structure by a brief, but, at the same time, careful inquiry into a number of specimens sufficiently numerous to warrant the deductions.

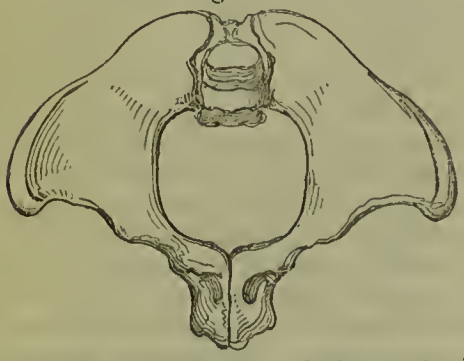
1st, In the skeleton of the adult fœtus now before me (not selected, but taken at random from the museum), prepared and dried in the usual way, the pelvis presents the following appearances. In its entire outline it resembles the pelvis of the quadruped; I mean of most adult mammals. It is more like an elongated canal than the human pelvis; the iliac walls are comparatively upright, the false pelvis contracted and narrow, falling exceedingly within the limits of a perpendicular line descending from the shoulders; a straight line directed horizontally from the symphysis of the pubis towards the vertebral column strikes the lowermost part of the coccygeal bones. The conjugate or antero-posterior diameter is $1\frac{2}{3}$ inch; the transverse, where widest, about 1 inch, and nearly of the same width throughout; in other words, the pelvis is no longer, or rather not yet, a transverse oval one, but quadrilateral, its greatest diameter, as in the brute, being from before backwards, and precisely the reverse of what it ought to become (to give it a strictly human form) in the adult. Already, in this fœtus, the coccygeal bones are twisted or curved

Fig. 1.



Outline of the pelvis of the human fœtus at term: the pelvis at this age may be taken as the type of the transcendental law; it is more quadrilateral than rounded or oval, and its antero-posterior diameter is the longest: it has the form, in a great measure, of the pelvis of the quadruped and quadrumanous mammal, of the human male pelvis generally, and of certain ill-formed female (all human) pelves.

Fig. 2.



Outline of the pelvis of the elephant: the introitus of the pelvis of the elephant is more rounded than that of most quadruped mammals, including even that of the quadrumana; still it does not assume the special transverse oval character of the well-formed human female pelvis.

Fig. 3.

Pelvis of the seal: this is the extreme of the type; this form occurs in the very young human fœtus, and I have seen an approach to it in the adult human male and even female: it is in this pelvis, in the adult female, that a separation of the pubic articulation takes place to so great an extent prior to and during the birth of the young seal.

Fig. 4.

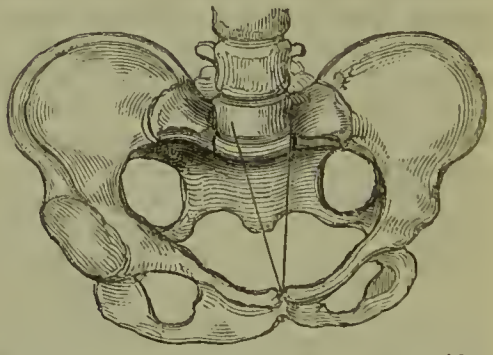
That form of the human female pelvis which I have called the quadruped form; it is quite common in this country, and I think in Europe generally; this is merely the fœtal form persisting, the transcendental law, or law of general type, prevailing over the law of species. Beauty of form, and fitness for the due performance of function, or the law of species, when not interfered with, gives rise, in woman, to the pelvis whose outline is represented by

Fig. 5.

This is the transverse oval pelvis: when to this is added a fully developed upper or false pelvis, then the osseous girdle is considered as completely or fully developed.

The transcendental law, or law of type, may prevail on one side of the pelvis, and not influence the other; this gives rise to the pelvis oblique ovata of Naegele, as seen in

Fig. 6.



When it takes place on both sides, as is said to be the case with a human female pelvis now in the possession of Dr. Outrepont, then the outline of the introitus will strongly resemble that of the seal—or, in other words, the transcendental of the very youngest period of the human skeleton, when, in fact, it was almost entirely cartilaginous, had continued in force to mature years, and the antagonistic law of species had, from causes unknown to us, never come into action. I should not think that the curvature of the spine in the lumbar region, seen in Fig. 6, was a necessary consequence of the non-development of the half of the pelvis.

towards the right side, and the area of the abdominal strait, or entrance, if divided into halves by a line passed across it as mesially as possible, gives to the right side of the area a capacity greater than the left; the left wall is straighter, and already the tendency to the pelvis oblique ovata of Naegele is established. Now, we have only to suppose the persistence of these forms, to a certain extent at least, to the adult age, and we then have the exact description of the ill-formed female pelvis so common in this country, and, I have no doubt, also in England; and if what travellers say of the Esquimaux be true, it would seem that the false pelvis is seldom or ever in them properly developed, so that the breadth of the haunches is nearly equal in both sexes; and this is quite possible, for the false pelvis has an entirely different and independent development from the true pelvis.

It would appear already, from the examination of a single specimen, that the more common of the malformations and peculiarities in the adult male and female pelvis are merely the persistence of the fœtal, infantile, or juvenile forms, and of consequence repeated in the pelvis of many of the mammalia. The obliquely oval pelvis of Naegele is comprised in the same law, with the addition of an extremely imperfect development of one side. But there is nothing new in it; nothing unexpected—nothing which might not, *à priori*, be foretold by an inspection of the fœtal skeleton.

I have already shewn, in a memoir published some years ago on the Statistics of Hernia, that the pelvis was very frequently more fully developed on one side than on the other, and hence the predisposition to hernia on the more fully developed side, be it right or left; but from the slightest shade of difference to the most deformed of the pelves lithographed by Naegele one law prevails—the persistence of the fœtal form on one side; the regular development of adult human form on the other. Again, the quadrilateral-formed pelvis is the type of most mammals (if not of all) when adult, and, by the law of unity of organization in the embryo, it is also the type of the early human pelvis. Whatever form, then, the pelvis assumes, must flow from the agency of these causes, and be connected with, and dependent on, these laws. Obliquely oval pelvises are not *lusus naturæ*, no more is the occasional quadrilateral form of the same girdle in the European female.

2. Another human fœtus at full term is put before you: the measurements and form of the pelvis are as nearly as may be those of the preceding: still there are differences: the iliac portion or false pelvis is broader, and although the whole length of the skeleton be less than the preceding one by at least an inch, the false pelvis is broader by nearly half an inch; confirming what we already know so frequently to happen, that the future grown man or woman may be shadowed out in the infant.

3. Another fœtal pelvis gives a conjugate diameter of an inch and $\frac{3}{8}$; the transverse diameter where widest scarcely measures an inch.

4. Here is the pelvis of an infant seemingly about a year old; the antero-posterior diameter of the pelvis exceeds the transverse by nearly half an inch.

Let us now examine a few adult female pelvises from amongst those whose dimensions and general form offer no obstacle to the ready passage to the child through their cavities: the first I take up gives a conjugate diameter of $4\frac{1}{2}$ inches: its transverse measures 6, and yet this pelvis is not equally developed: but it is a large and capacious pelvis, and the true as well as the false have very fully assumed the human character; it is the very reverse of the infantile and brute pelvis in every respect, in its form, its dimensions, and inclination.

The next I examine is different, and yet a sufficiently capacious pelvis; first, the false pelvis is narrow and the walls upright; the woman, therefore, wanted breadth of haunches. No proof, however, of a want of capacity in the true pelvis itself, as this follows different laws in its development. The conjugate diameter measures $4\frac{1}{2}$ inches, the transverse $5\frac{1}{2}$, or nearly so, being at least half an inch less than the preceding pelvis: but still it is a good pelvis compared with some which are to follow. The next measures $4\frac{2}{8}$ and 5, and is becoming more and more at its abdominal entrance (I mean the introitus) of an elongated square form. The next is as nearly as may be 4 inches each way, without any appearance of rickets, and

the same precisely may be said of another standing close to it. Now all these are female pelvises presenting no appearance of any disease or deformity; I have so far selected them on this account. But here are others which shew these phenomena equally, or even more so, but being complicated with other peculiarities, had better follow the more simple ones.

The sacrum I now examine shews the following peculiarities: it is that of a female: the right half of the sacrum is nearly half an inch narrower than the left; the right half of the introitus proportionably smaller, but the iliac portion of the ossa innominata are tolerably symmetrical. Here, then, is the commencement of the pelvis oblique ovata of Naegele. The lumbar portion of the column presents an extensive curve, the cause of which curve it would be more difficult to investigate, but which probably arose from the same predisposition to a want of symmetry. A second pelvis also, in Dr. Campbell's museum, presents the obliquely oval pelvis on the left side; a large exostotic deposition on the last lumbar vertebra rendering, I should think, the passage of any living child impossible. And now I take the male pelvis, the one before me, and I find it to resemble in the form of the introitus, as nearly as may be, that of the seal, and of many other quadrupeds, although the transverse diameter be still somewhat larger than the conjugate: a second male pelvis, now in my own museum, shews the brute form to a still more extraordinary degree. The transverse diameter of its widest part is but about $\frac{4}{8}$ larger than the antero-posterior; and, precisely as in that of the quadruped, it is greatest close to the sacrum, scarcely widening, or but very little, anterior to this. Now this is the type of the fœtal pelvis, and that of the mammalia lower than man; and that this person presents a pelvis of this peculiar shape, is simply because it was the form it originally had, and from which, by a peculiarity in his nature, it had never altered.

But this male pelvis merits a further notice. The false pelvis spreads out after the fashion of the female pelvis; the true pelvis is constructed on the plan of the fœtus and of the quadruped mammal. The sacrum is of such breadth that immediately in front of it, or very nearly so, we find the widest part of the introitus; that is, the edges of the abdominal entrance run nearly straight down, and then converge. The last lumbar vertebra has evidently united by its lateral processes with the corresponding parts of the sacrum, so as to become identified with it, causing it strongly to resemble the first sacral vertebra.

I shall now endeavour to apply these views derived from transcendental anatomy to the practical work of Naegele, and to his discovery of the pelvis oblique ovata. From time to time there occur, no doubt, in every dissecting room, specimens similar to the five or six now lying before me; they have been in my own museum nearly seventeen years, and have annually been exhibited to my class. I shall describe one of them with some minuteness, the rest more briefly.

1. A female pelvis, history unknown. The right os innominatum is not present; the left had been accidentally divided with the saw just before the acetabulum, and the fragment lost; the last lumbar vertebra is present; the sacrum also, and seemingly the first of the coccygeal vertebræ. To understand the preparation rightly, it requires to be seen, or to place Mr. Naegele's work before you. The left sacro-iliac articulation is obliterated or anchylosed; the iliac portion of the left innominatum small, and but partially developed. The sacrum is twisted away to one side; the area of the left half of the introitus could scarcely have equalled a third of the right: the specimen when entire must have presented exactly the form we find in M. Naegele's plate, and have been one of the very best specimens of the obliquely oval pelvis.

2. The preparation marked 980 is similar to the last in nearly all respects: the partial development has again happened on the left side; the anchylosis of the joint is complete only on the surface, but the obliquely oval form would not have been so well marked as in the preceding specimen.

3. In the specimen now before me, which is an extremely instructive one, as shewing the early stage at which these deformations commence, the pelvis being evidently that of a young person, a female, the defective development had taken place on the right side: there is no appearance whatever of any anchylosis,—a change which seems to

come on later, and at times to be altogether unconnected with the deformity. The first sacral vertebra strongly resembles the last lumbar; it has not yet united with the second, and the two lateral portions or wings are of extremely unequal depth; the one, the right, being scarcely half an inch; the other, the left, being fully an inch and $\frac{1}{8}$; neither does the body of this vertebra follow so exactly the direction of the second as is usual in the regular-formed sacrum, but slopes slightly backward; lastly, a line let fall perpendicularly from the symphysis pubis to the sacrum strikes that bone at the inner edge of the right foramina vertebralia, instead of the centre of the bone, as it ought to do. On looking at the specimen when held fairly before you, you may see nearly the whole of the right foramina vertebralia, which ought not to be the case had the right os innominatum been fairly developed; but instead of that semicircular line which the linea innominata ought to present, we find it nearly a constantly converging one from the articulation with the sacrum to the symphysis pubis. No one, I think, can now doubt what would have been the ultimate form of the pelvis had the person happily lived to mature years: it would have been a perfect specimen of the pelvis oblique ovata, the right side retaining the infantile and brute form, the left expanding into that of the well-formed woman.

I shall now very briefly notice three more specimens which I find on the same shelf with those just described, but evidently quite different in their nature; I notice these the more particularly, that their characteristic pathological condition may have orginally led others as well as myself to overlook the essential differences betwixt them and the preceding specimens. They are half sections of the pelvis taken at random from the Practical Rooms, exhibiting an anchylosis of the right sacro-iliac joint—no other deformity is present; the pelves shewing no marks of deformation. One of the persons had probably suffered from rheumatism; at least other parts of the bones shew a strong disposition to exostotic deposits, which indeed abound. Thus anchylosis of this joint is in no shape necessarily connected with the pelvis oblique ovata, although it usually follows or accompanies it as a consequence of non-development.

The last specimen on the table reminds me of the vast variety of forms presented by the sacrum, and how difficult it is to give even the characteristic sexual distinctions. Here is one which would easily admit of its first bone being nicely removed with the saw, and yet leave a perfect sacrum, that is, a promontory and five bones. There appear to have been six sacral vertebræ in this specimen, but as neither the lumbar nor coccygeal bones are present, the matter remains doubtful. Another and a perfectly different view might be taken of this upper vertebra: it may be the last lumbar, but I feel disposed to say that, notwithstanding its anomalous appearance, it is the first sacral, and that the first coccygeal is here united to the last sacral.

As these observations have unexpectedly extended so much beyond the limits I anticipated, what I have further to say shall be very brief; it has a reference to two points: 1st, to some observations by Fischer and Autenrieth on the form of the pelvis of the quadruped mammal; 2d, as to the temporary relaxation of the pelvic ligaments in women preceding and during delivery.

1st, In addition to a vast number of very admirable and original observations, Autenrieth remarks, that the depth of the osseous symphysis of the pubis, and the comparative weakness of the sacral and caudal portions; the canal-like shape assumed by the pelvis in most of the lower animals; the rudimentary condition of the false or upper pelvis in some, and the like condition of the true pelvis in others; all these, he observes, are remarkable features in the pelvis of the quadruped. The influence of these changes is perceived in the condition of the muscles connected with these bones, in the comparative slenderness of the haunches of most quadrupeds, and the frequency of hernia in man as compared with them. The usual vertical position of the trunk in man has, no doubt, its share in giving rise to this circumstance. Autenrieth's mode of calculating certain dimensions of the pelvis is most ingenious, but to do justice to his ideas it would require that the work were translated and transferred to your pages entire: he divides the pelvis, for example, into what he calls

a dorsal part and an abdominal, and he found that, whilst in the infant of two years the first was to the second in length as 10 to 11, and up to 14; in the adult it was as 10 to 16, up to 22.

It is sufficiently curious that, although in certain quadrumana, and in the elephant, the comparatively rounded form of the cranium in this respect somewhat, though rather remotely in the latter, resembling the human, might have been supposed adequate to produce a marked alteration in the form of the pelvis, it yet has not done so, at least not to any great extent. The pelvis of the highest of the quadrumana does not, in so far as I recollect, approach in form the female (human) pelvis; and in respect to the elephant, although it may be admitted to be more rounded in the *introitus*, or *narrow*, than most mammal pelves, its resemblance to the transversely oval human female pelvis is very distant.

2d, Do the articulations of the female pelvis relax during, and a little before, delivery? To what extent does the relaxation take place? Is it a rare or frequent occurrence? Is it a pathological or a healthy condition?

To answer these questions in as far as my own experience goes, I shall endeavour to relate, to the best of my recollection, some of the principal facts bearing on them in the order in which they presented themselves to me.

During the summer sessions of 1825, 1826, and 1827, I delivered, in Surgeons' Square, three courses of lectures on comparative anatomy. The desire to present as many fresh specimens of all the great classes, and principal genera, of animals, induced me to apply to the fishermen on our coasts, engaging them to send me any rare mollusca fishes, seals, dolphins, &c. which they might take by nets or otherwise. Now, under those circumstances, a pregnant seal was brought me; the animal had been shot with a single ball, killing not only the mother but the fœtus. From the size of the young seal and other circumstances, it was evident that in a very few days the fœtus would have been born. I shall here abstain from mentioning any other circumstances respecting the anatomy of the seal and its fœtus, saving what is directly connected with the history of the pelvis—I mean of the pelvis of this pregnant seal. The fœtus, which lay in the uterus with all its natural connexions, appeared to me of extraordinary dimensions, and I was curious to know how such a fœtus could pass the pelvis of the mother: the examination of this cavity displayed a singular and unexpected fact, and which was at the time quite new to me; the interpubic ligament, or ligamentous symphysis, was so elongated that the bones might be separated from each other to the extent of nearly two inches. When I now examined attentively the form of the osseous pelvis, I could see very evidently that in the quadrilateral-shaped pelvis, where the greatest diameter was the antero-posterior one, as in the quadruped mammal, and not the transverse one, as in woman, this temporary separation of the pubic bones of the pelvis admitted of a great enlargement of all the diameters of the pelvis, whilst in the transversely oval pelvis of woman, the artificial separation of the pubic bones merely, even to a considerable extent, say an inch, did but little increase any of the diameters of the brim and abdominal entrance to the true pelvis. As this was a matter which could be made the subject of comparative demonstration, several female pelves were divided at the pubes, as recommended by Sigault, when it became manifest to the class that unless the pubic bones were separated forcibly from each other for more than an inch and a quarter, nothing was gained on the diameters of the cavity; and that a more extended separation, so as to render the operation at all useful in respect to its object, was absolutely impracticable on the living woman, since, on its being attempted, the sacro-iliac joints were heard to give way. Having satisfied myself that the Sigaultian operation on the transversely oval pelvis of woman could lead to no real benefit, being only applicable to that form of pelvis found in the quadruped, and but rarely in woman, my attention was next directed to ascertain whether or not, in woman, the ligamentous symphysis of the pubis became relaxed during parturition? Whether or not this was frequent or rare? Whether or not it was accompanied by a corresponding relaxation of the sacro-iliac joints, without which the relaxation of the ligamentous symphysis would be of little avail? And, lastly,

whether such relaxations were to be viewed as pathological or natural? An opportunity soon occurred, after the dissection of the seal described above, to put some of these questions to the test of direct observation.

A middle-aged woman, who had died of flooding following delivery, was brought into the Practical Rooms: the pelvis was of full dimensions. On examining its articulations, they were found to be all relaxed; the bones could be made to slide over each other. The dissection was shewn to the class, and compared with that of the seal, whose skeleton was in the museum; the opinions, likewise, of several distinguished accoucheurs in town were asked: they all declared the relaxation to be the effect of one of two causes: it was either, said they, the result of putrefaction after death, or of a diseased or pathological condition of the pelvis. On consulting the published works of a most distinguished surgeon and accoucheur (Mr. Burns, of Glasgow), I found that he also maintained the doctrine, that when relaxation of the articulations of the pelvis did take place during delivery, it was a pathological and not a healthy process.

Since 1825 and 1826, I have now had an opportunity of examining carefully the pelves of five women of different ages who have died soon after delivery, and having found in all of these a relaxation of the articulations of the pelvis to a greater or less extent, but always remarkable, I feel disposed to think the process a regular or healthy one, and not pathological.

In conclusion, I trust it has been made apparent that the great laws of transcendental anatomy and physiology, even admitting them to be not yet very fully established, are yet extensively and happily applicable to human anatomy and physiology: certain deviations of the pelvis, for example, from its normal and specific form, have been shewn to be *merely a persistence of its fœtal shape;* and this shape again, being the *type of shape in every mammal*, the deformation gives to the adult pelvis thus constituted the shape at once of the human fœtal and quadruped pelvis. Or, to express the generalization as a law, perhaps we may say, "the laws regulating the growth of *specific forms* are the antithesis of the laws presiding over *transcendental forms;* the one bestows individuality on the species, the other struggles to reduce all to one type; as the one prevails, the specific form is preserved; with the predominance of the other, a destruction of all *speciality* exists: we call these laws of type deformating laws, because they are opposed to our ideas of species, and to the obvious endeavours of nature to maintain this struggle and to perpetuate species.

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